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COLLEGE OF AGRICULTURE AND HOME ECONOMICS
AGRICULTURAL EXPERIMENT STATION

DEPARTMENT OF ANIMAL PATHOLOGY

LEXINGTON, KY.

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Dear Doctor Lederberg: I have intended answering your letter of Aug 26 since the day it arrived but pressure of other matters intervened. I have one some things I want to say in regard to the contents of your two letters. In reading them I am sure you will keep in mind that they are not written by one familiar with genetics nor its literature, even as it regards bacteria. Nevertheless, there are some things that I feel called upon to say.

Bacteria are extremely adaptable and can be coaxed to do many things and I do not believe that genes or selection enter into all the adaptive changes that can be demonstrated. I do not pretend to know what lays behind the development of an adaptive enzyme but I do not believe it is simple selection nor does hybridization always enter into it. For instance, in this laboratory Salmonella cultures repeatedly reisolated from single colonies have been trained to ferment lactose and/or sucrose. In some of these cultures the acquired properties were soon lost, even though single colonies which exhibited the acquired character were selected repeatedly. ~~In other cultures~~ The acquired character was soon lost after the substrate in question was withdrawn. In other instances the organisms retained the acquired character through repeated transfers in the absence of the fermentable substance. Further, bacteria can be trained to utilize numerous other substances, and the Enterobacteriaceae are notorious in this respect. If I am not mistaken numerous adaptation of phage to certain strains has occurred in which hybridization apparently played ~~no~~ no part.

In regard to Salmonella phages, probably Dr. M. H. Rakieten, 1005 S. Oxford St., Brooklyn has more than anyone else, as did I before went to the Army. I have not talked to him about them since his return.

Your requirement of a strain that is "almost entirely

(over)

monophasic, but in which the alternative phase can be detected etc." does not include too many types. Probably S. thompson var. berlin (VI, VII : [76] - 1,5...) best meets it. If you want some cultures I can send both VI, VII ; R - and VI, VII ; - 1,5... cultures and the corresponding serums.

Induced variation - well, I don't know. There is good evidence that the variations actually are induced in certain instances but of course it is a question which could be argued endlessly. Some cultures on changes with relative ease but once they are changed are extremely resistant to reversion. In other instances growth in serum produces antigens never observed in nature. In other cases changes can be brought about by one serum but not by another of apparently the same agglutinin content - cattle would work equally well in a variable culture. I think some influence must be attributed to the action of the serum, hence induced variation. The well known process of roughening cultures by growth in homologous serum is the most obvious example.

One more remark and I am through - In our experience change of phase in an organism which does not vary readily does not lead to selection of a diphasic culture but to a form more stubbornly monophasic than the original. I think this is a point you should keep in mind when you employ monophasic cultures in your work. Incidentally, Salmonella are not the only bugs which exhibit natural phase variation - we have a group of diphasic paracolon bacilli that are really beauts for a mixed up mess

Finally, I think we are writing at cross purposes and do not understand each other. A long conversation would most certainly be helpful but I see no opportunity of coming East soon. My only trip this winter will be to Chicago in early December. There are some geneticists meeting there. Wouldn't be going would you?

Sincerely
P. R. Edwards